Vol. 34, No. 5 May, 1937

THE

PSYCHOLOGICAL BULLETIN

AN EVALUATION OF THE ATTEMPTS TO MEASURE SOCIAL INTELLIGENCE

BY ROBERT L. THORNDIKE AND SAUL STEIN

Teachers College, Columbia University

In 1920 E. L. Thorndike (20) suggested his frequently quoted division of intelligence into abstract, mechanical, and social intelligences. Abstract intelligence was spoken of as ability to understand and manage ideas and abstractions, mechanical intelligence as ability to understand and manage the concrete objects of the physical environment, and social intelligence as *ability to understand and manage people.** A great many tests have been developed to measure abstract intelligence, a fair number to measure mechanical intelligence, but very few to measure social intelligence. It is this last small group of tests which this article will treat.

It is difficult to delimit exactly the area which should be included in a discussion of social intelligence. We will first of all indicate 3 areas which seem to be adjacent to social intelligence, perhaps related to it, and often confused with it. Brief mention will be made of a few tests in these areas. Then the discussion will proceed to tests of social intelligence proper.

Measures in Areas Allied to Social Intelligence

There have been developed during the last 15 years a great many measures of aspects of the individual which are more or less closely allied to social intelligence as it has been defined above. They are not to be identified with this social intelligence, though further investigation may show them to be related to it. They may be roughly divided into 3 groups: (1) those dealing with a different meaning of the word "social," (2) those attempting to assay

* Italics by present authors.

interest or attitude, and (3) those measuring items of acquired information.

There are a great many tests of the "social" reactions of the individual, where social is used in the sense of "pertaining to society." These are tests of the reaction of the individual to the more or less institutionalized phases of society. We may include here measures of attitude on economic and political issues, tests of fairmindedness and honesty—in fact almost all of that great range of measuring instruments which are classed as tests of "character." These are all concerned with the adequacy of the individual's understanding of and response to some phase of "society" as society—adequacy defined from society's point of view. They are not primarily concerned with the reaction of individual to individual. We find them reviewed in the report of the Character Education Inquiry (9) and periodically in the PSYCHOLOGICAL BULLETIN.

A second large group of studies is concerned with social interest, attitude, and adjustment. The measuring instruments here usually take the form of questionnaires. Practically all of the introversion-extroversion questionnaires contain items on the social adjustments of the individual, and the importance of this phase of adjustment has been emphasized in the recent development by Flanagan (4) of a "sociability" scoring key for the Bernreuter Personality Inventory and by Guilford (8) of a social introversion key for the Nebraska Personality Inventory.

Washburne (22) has prepared a questionnaire concerned directly with the measurement of factors in "social adjustment." The questionnaire includes sections designed to measure purpose, socialness, sympathy, poise, and impulse-judgment. The phases of the questionnaire were selected on an *a priori* consideration of the factors entering into social adjustment, and were validated by using matched pairs of adolescents, selected for their good or poor social adjustment. The reliabilities of the sections of the questionnaire are reported to range from .73 to .88 and the reliability of the whole to be .92. The total test, and most of the sub-tests, were found to differentiate between the well- and poorly-adjusted. Comparing a well-adjusted high school group with a group of prisoners and delinquents of comparable age, Washburne reports a bi-serial r of .90 as evidence of the validity of the test. It must be remembered in this connection, however, that this correlation coefficient is markedly exaggerated by considering only the 2 extremes of the distribution of adjustment.

Questionnaires directed more specifically toward measuring "sociability" are those of Gilliland and Burke (6) and of Stauter and Hunting (15).

Gilliland and Burke tried out several measures which they had "devised to measure the social intelligence or sociability of the individual." They used the 2 terms as synonyms, and in their definition included both "ability to get on equably with his fellow men" and "fond of mingling with others," making no distinction between the 2 aspects of ability on the one hand and inclination on the other. They used 3 tests of memory for faces and a short questionnaire on range of friends, number of social outings participated in, and degree of liking for several social activities. The tests of memory for faces will be considered later. The reliability of the questionnaire is not given. Its validity, as determined by correlation with the average of ratings for sociability given each other by members of a psychology class, was .60, .50 and .43 in 3 different classes. Considering the fallible nature of the criterion, it seems as if this approach to measuring "sociability" is quite promising. However, the identity of sociability and social intelligence has not been demonstrated.

Stauter and Hunting (15) have systematically extended one aspect of the Gilliland questionnaire, and have prepared a questionnaire which tries to get a more complete picture of width of acquaintance. Acquaintanceship is defined as each knowing the other's name. The questionnaire asks for the number of one's acquaintances in a wide variety of specialized groups (as Elks, Nurses, Filipinos, etc.). From this a total acquaintanceship score is obtained. The reliability of the test is reported as .918. It was validated by comparing those who had received mention for non-academic and non-athletic activity in the school annual or newspaper with those who had not. Differences were from 5 to 8 times their standard error.

None of the 3 tests which we have just considered appears to have any appreciable correlation with abstract intelligence. Whether they have any relationship to "ability to deal with people" also remains to be determined. There seems to have been no convincing attempt to determine this relationship.

The third group of measures which we have mentioned above—information tests—tend to coalesce with the first group, and we find many tests of information about society. Tests of ethical vocabulary, knowledge of different sports, knowledge of government and customs are typical of these. A test of more intimate and personal

social information is reported by Strang (**17**). This Test of Social Usage for high school students covers those aspects of personal-social contacts which are included in manners and etiquette. It is found that scores in the test increase steadily from the seventh to the twelfth grade. The test also differentiates those students whose parents fall in different occupational classes. With abstract intelligence it correlates to the extent of .40 or .50. It shows no consistent relationship to school activities.

Sub-tests of information have also been included in those tests which purport to measure social intelligence. The first form of the George Washington Social Intelligence Test included a sub-test of social information. This has been dropped from the most recent form. A test of social intelligence reported in preliminary form by the Bureau of Public Personnel Administration also contains information items.

Preliminary Attempts to Measure Social Intelligence

There have been several tentative approaches to the measurement of social intelligence which have been presented in a single experiment and followed no further. Gilliland and Burke (**6**) tried out, besides their sociability questionnaire, 3 tests of picture recognition. Pictures of individuals were studied, and then presented for recognition among a larger group of pictures. These tests tended to show some relationship to class ratings for sociability, but the correlations were lower than those of the questionnaire. The reliabilities of the tests were not given. They showed little correlation with one another.

The Bureau of Public Personnel Administration (**3**) reports a preliminary form of a paper and pencil test of social intelligence. The test is composed of 100 true-false and multiple choice statements about types of people and behavior and 50 descriptions of social situations with choices for courses of action. No data concerning reliability, validity, or relation to other variables are given.

Reed and Weidemann (**14**) describe a Social Situation Judgments Test, in which the subject is required to state the extent of his agreement or disagreement (on a five-point scale) with a series of statements concerning social behavior or judgments of social behavior. A scoring key was suggested, with credits awarded in proportion to the per cent of individuals giving each answer. Scored in this way the test has a reliability of about .80. No effort was made to establish the validity of the test as a measure of anything. Scores based upon this social norm correlated with an intelligence test .16 and with an English classification test .30.

No one of the experiments reported in this section has been followed up thoroughly enough to make it possible to evaluate its contribution to the measurement of social intelligence.

The George Washington Social Intelligence Test

The one test to measure social intelligence that has been widely distributed and widely studied is the George Washington Test of Social Intelligence. This test was first prepared by F. A. Moss and his associates at George Washington University in 1926. It has subsequently been revised, though the main outline of the test has not been changed. This test is a paper and pencil test, and has contained the following sub-tests:

1. *Judgment in social situations*—a multiple choice test of problems in social relationships.
2. *Recognition of mental state of speaker*—a matching test of short speeches and names of emotions.
3. *Observation of human behavior*—a true-false test of generalizations about human behavior.
4. *Memory for names and faces*—a matching test where names and faces previously studied are identified among a larger number.
5. *Sense of humor*—a multiple choice test of completing jokes (in revised form only).
6. *Identification of emotional expression*—a matching test of pictures and names of emotions (in original form only).
7. *Social information*—a true-false test of information about matters of social interest (in original form only).

Because of its unique position as a widely known and widely distributed test purporting to measure an important aspect of the individual, this test has wisely been subjected to a considerable amount of study and criticism.

Reliability of the George Washington Social Intelligence Test. Hunt (10) reports a correlation of .89 when the test was given to a group of 100 college sophomores a second time after a lapse of 4 months. She also reports a correlation of .88 for a group of 129 college students, obtained by correlating odd *vs.* even and correcting by the Brown-Spearman prophecy formula.

Validity of the George Washington Social Intelligence Test. Three types of evidence have been presented by Hunt (10) to establish the validity of the test.

(1) Marked occupational differences were found in the test scores. Executives, salesmen, and teachers made high scores, while clerks and unskilled laborers made low scores. There seemed to be

a correspondence between the social demands of the occupation and the scores on this test, though the question has been raised as to whether teachers should be expected to show up well on a test of social intelligence. In connection with this type of validation, it should be mentioned that no effort was made to partial out abstract intelligence, and that occupational groups are ordinarily found to be differentiated by tests of abstract intelligence in much the same way that the social intelligence test differentiated them.

(2) Hunt found a correspondence between scores on the test and number of extracurricular activities engaged in by college freshmen. The median scores for different numbers of activities are given in Table I.

TABLE I

Activities	Median Score
4 or more	116
3	112
2	110
1	105
0	99

In contrast to these results, Strang (19), working with a group of 311 graduate students at Teachers College found no significant relationship between test scores and participation in club activities. The correlation was .11. It should perhaps be remarked in this connection that the distribution of activity scores is very coarse and markedly skewed. This raises some question as to the applicability of the coefficient of correlation to the data. The selection of individuals and activities included in the 2 studies was also quite different.

(3) Hunt found the correlation between social intelligence scores and rating by a superior executive (who had good opportunity to know their ability in dealing with people) for 98 employees in a large sales company to be .61. Of those making above average scores, 75% were above average in ability to get along with people as rated by the judge, and of those making below average scores 77% were below average on the rating.

McClatchey (12), on the other hand, found no appreciable difference in test score between a group of college girls who were selected as making the best social adaptation of any in their sorority and an unselected group of college students. Within the selected group, the correlation between rating for social adaptability and test score was .16. Here again we must take into account the difference between the groups and situations involved in the 2 studies.

Criticisms of the George Washington Social Intelligence Test.

Over and above the negative results of Strang and McClatchey reported above, 2 chief lines of criticism have been directed at the social intelligence test: (1) that it does not correlate with other tests of the social side of the individual, and (2) that it shows relatively high correlation with tests of abstract intelligence.

The social intelligence test has been correlated with the Gilliland sociability questionnaire by Pintner and Upshall (13) and by Strang (19). The correlations were respectively .14 and .17. Though the reliability of the questionnaire is not known, it does not seem likely that correction for attenuation would raise these correlations markedly. It seems evident that the two measures are not of the same thing. However, this is not necessarily to be understood as a criticism of the validity of the social intelligence test. It may be that interest in people and ability to understand and manage people are not closely related.

McClatchey (12) reports a correlation of .53 between the social intelligence test and the Colgate Introversion-Extroversion Personal Inventory for a group of 27 college students, and expresses surprise that a test of social intelligence should be thus related.

A number of experimenters have investigated the relationship of the George Washington Social Intelligence Test to tests of abstract intelligence. The results are summarized in Table II.

TABLE II

CORRELATIONS OF THE GEORGE WASHINGTON SOCIAL INTELLIGENCE TEST WITH MEASURES OF ABSTRACT INTELLIGENCE

General Intelligence Test Used	r	Sample	Investigator
George Washington Mental Alertness Test	.54	243 College Freshmen	Hunt (10)
O'Rourke College Entrance Test	.57	102 College Sophomores	Hunt (10)
Brown University Test	.56	96 College Freshmen and Sophomores	Hunt (10)
McCall Multi-Mental Test	.25	130 Advanced Students	Hunt (10)
Thorndike Intelligence Examination for High School Graduates	.58	258 Graduate Students	Broom (2)
Same	.42	118 College Freshmen	Garrett & Kellogg (5)
Terman Test	.75	136 Senior High School Girls	Grosvenor (7)
Thorndike Intelligence Test for College Students	.68	33 Graduate Students	Pintner & Upshall (13)
George Washington Mental Alertness Test	.69	500 College Freshmen	Stein (16)

In considering these correlations, we must remember that they are raw correlations, and will be raised somewhat when corrected for attenuation. It should also be noted that most of the groups studied were quite select groups of college or graduate students, and that we should expect higher correlations with more heterogeneous groups. The median of the reported correlations is .57. In view of the factors just mentioned, it seems safe to say that there is a marked overlapping between the social intelligence test and tests of abstract intelligence.

This was demonstrated even more conclusively in the work of Stein (16). He computed the correlation of the total tests and all the sub-tests of the George Washington Social Intelligence Test with the George Washington Mental Alertness Test. These correlations are shown in Table III.

The average intercorrelation of parts of the Social Intelligence Test was .335. The average intercorrelation of parts of the Mental Alertness Test was .390. The average of the correlation of sub-tests of one test with sub-tests of the other was .344. This suggests that there is about as much in common between the 2 tests as there is within either.

A further check on the community of these 2 tests was made by R. L. Thorndike (21). Applying Thurstone's center of gravity method of factor analysis to Stein's data and additional results from another similar group, he was able to obtain a satisfactory fit to the correlation matrix with a pattern of 3 factors. Of these, the first factor was overwhelmingly the most important; it had good-sized positive weights for every variable, and seemed heavily loaded with verbal ability. This corresponds to the opinion expressed by Broom (2) that verbal ability accounts for the community between the George Washington Social Intelligence Test and tests of abstract intelligence. The second factor gave some evidence of differentiating the social from the abstract intelligence test, but the 2 were not sharply separated. The third factor seemed to be a factor of speed.

All in all, these results seem to confirm E. L. Thorndike's original statement, made when he first suggested the existence of social intelligence, that " when the mind works with general facts *about* things and people . . . its action is referred to abstract intelligence." We are forced to conclude that the George Washington Social Intelligence Test is so heavily loaded with ability to work with words and ideas, that differences in social intelligence tend to be swamped by differences in abstract intelligence. There is no conclusive evi-

TABLE III

CORRELATIONS BETWEEN THE GEORGE WASHINGTON UNIVERSITY SOCIAL INTELLIGENCE TEST AND THE GEORGE WASHINGTON MENTAL ALERTNESS TEST

	S.I. score	1	2	3	4	5	M.A. score	6	7	8	9	10
Social Intelligence Score	....	.466	.626	.828	.548	.636	.692	.669	.339	.447	.101	.545
Judgment in Social Situations (1)	.466	....	.362	.422	.242	.330	.481	.436	.333	.328	.228	.412
Recognition of Mental State of Speaker (2)	.626	.362	....	.473	.257	.327	.434	.462	.291	.359	.151	.351
Observation of Human Behavior (3)	.828	.422	.473	....	.243	.364	.567	.585	.375	.446	.166	.457
Memory for Names and Faces (4)	.548	.242	.257	.243	....	.329	.329	.323	.148	.291	.088	.249
Sense of Humor (5)	.636	.330	.327	.364	.329	....	.482	.416	.312	.353	.202	.449
Mental Alertness Score	.692	.481	.434	.567	.329	.482	....	.817	.728	.791	.398	.624
Vocabulary (6)	.669	.436	.462	.585	.323	.416	.817	....	.543	.604	.231	.491
General Information (7)	.339	.333	.291	.375	.148	.312	.728	.543	....	.344	.311	.254
Learning Ability (8)	.447	.328	.359	.446	.291	.353	.791	.604	.344	....	.308	.410
Arithmetical Reasoning (9)	.101	.228	.151	.166	.088	.202	.398	.231	.311	.308	....	.408
Comprehension (10)	.545	.412	.351	.457	.249	.449	.624	.491	.254	.410	.408	....

dence, when these differences in abstract intelligence are allowed for, that the test has any validity as a measure of social intelligence.

Discussion and Conclusions

A survey of the experimentation on social intelligence leaves one with the conclusion that "ability to deal with people" has not been satisfactorily measured. The George Washington test has not stood up well under experimental examination, and until further research demonstrates that it is anything beyond a rather poor test of general intelligence, it must be looked upon with suspicion. No other test of this "ability" has been explored sufficiently for any significant conclusions to be drawn.

Whether there is any unitary trait corresponding to social intelligence remains to be demonstrated. It may be that when the contributions of abstract intelligence (or of various of the factors which make up abstract intelligence) and of interest in people are removed there will be nothing left. It may be that social intelligence is a complex of several different abilities, or a complex of an enormous number of specific social habits and attitudes. In any event, one is left with the feeling that the present approaches are of very limited value in determining the ability of an individual to react satisfactorily to other individuals.

It seems doubtful whether any test which is predominantly verbal can measure social ability. We hope that further investigation, via situation tests, movies, etc., getting closer to the actual social reaction and further from words, may throw more light on the nature of ability to manage and understand people.

BIBLIOGRAPHY

1. Broom, M. E., Note on Validity of a Test of Social Intelligence. *J. Appl. Psychol.*, 1928, **12**, 426–428.
2. Broom, M. E., A Further Study of the Validity of a Test of Social Intelligence. *J. Educ. Res.*, 1930, **22**, 403–405.
3. Bureau of Public Personnel Administration, A Partially Standardized Test of Social Intelligence. *Pub. Person. Stud.*, 1930, **8**, 73–79.
4. Flanagan, J. C., *Factor Analysis in the Study of Personality.* Stanford Univ., Calif.: Stanford Univ. Press, 1935. Pp. 103.
5. Garrett, H. E., and Kellogg, W. N., Relation of Physical Constitution to General Intelligence, Social Intelligence, and Emotional Stability. *J. Exper. Psychol.*, 1928, **11**, 113–129.
6. Gilliland, A. R., and Burke, R. S., A Measure of Sociability. *J. Appl. Psychol.*, 1926, **10**, 315–326.

7. GROSVENOR, E. L., A Study of the Social Intelligence of High School Pupils. *Amer. Phys. Educ. Rev.*, 1927, **32**, 656.
8. GUILFORD, J. P., *Laboratory Studies in Psychology*. New York: Henry Holt & Co., 1934. Pp. 289.
9. HARTSHORNE, H., MAY, M. A., and SHUTTLEWORTH, F. K., *Studies in the Organization of Character*. New York: The Macmillan Co., 1930. Pp. 503.
10. HUNT, T., The Measurement of Social Intelligence. *J. Appl. Psychol.*, 1928, **12**, 317–334.
11. HUNT, T., Social Intelligence and Where to Find It. *Industr. Psychol.*, 1927, **2**, 603–612.
12. MCCLATCHEY, V. R., A Theoretical and Statistical Critique of the Concept of Social Intelligence and of Attempts to Measure Such a Process. *J. Abn. & Soc. Psychol.*, 1929, **24**, 217–220.
13. PINTNER, R., and UPSHALL, C., Some Results of Social Intelligence Tests. *Sch. & Soc.*, 1928, **27**, 369–370.
14. REED, A. S., and WEIDEMANN, C. C., A Social Situation Judgments Test—Form I. *Educ. Res. Rec.*, 1933, **5**, 10–17.
15. STAUTER, J. J., and HUNTING, L. M., An Acquaintanceship Questionnaire as a Test of Sociability. *J. Soc. Psychol.*, 1933, **4**, 377–380.
16. STEIN, S., *A Statistical Study of the George Washington University Social Intelligence Test and the George Washington University Mental Alertness Test*. Unpublished Master's Thesis at George Washington University Library, 1935.
17. STRANG, R., Knowledge of Social Usage in Junior and Senior High Schools. *Sch. & Soc.*, 1931, **34**, 709–712.
18. STRANG, R., Measures of Social Intelligence. *Amer. J. Sociol.*, 1930, **36**, 263–269.
19. STRANG, R., Relation of Social Intelligence to Certain Other Factors. *Sch. & Soc.*, 1930, **32**, 268–272.
20. THORNDIKE, E. L., Intelligence and Its Uses. *Harper's Mag.*, 1920, **140**, 227–235.
21. THORNDIKE, R. L., Factor Analysis of Social and Abstract Intelligence. *J. Educ. Psychol.*, 1936, **27**, 231–233.
22. WASHBURNE, J. N., A Test of Social Adjustment. *J. Appl. Psychol.*, 1935, **19**, 125–144.

FACILITATION AND INHIBITION IN MENTAL WORK[1]

BY ARTHUR G. BILLS
University of Chicago

The field covered by this title is a tremendously broad and heterogeneous one. It is almost as broad as psychology itself. Yet fortunately for the reviewer, there have emerged, in the last several years, a few well defined problems or issues around which cluster most of the contemporary research. It will be advantageous to indicate at the start what these major issues are.

One problem is that of the *mechanics of effort*. Just what happens in the sensori-neuro-muscular mechanisms when a person exerts effort in thinking, when he thinks intensely? What is it which enables him to hold his "attention"—to use a traditional mode of expression—to a difficult or boring task in spite of strong inclinations to the contrary? What is the rôle of muscular tension, proprioceptive impulses, neuromuscular sets, and the like?

This first problem is quite independent of a second problem, that of motivation, which deals with the *dynamics of effort, i.e.* the question of why a person *wants* to exert effort, why he sets in motion these mechanisms of reinforcement about which we speak. This second problem deals with such factors as incentives, emotional drives, and the like.

A third problem is concerned with the effects of extraneous or adventitious stimuli on mental performance. How do so-called distractions in the environmental background affect the level of performance?

The fourth and last major problem is that of the facilitative and inhibitive effects of mental performance on itself. This includes the effects of preceding on subsequent performance, as well as the reciprocal interactive effects of operations carried out simultaneously.

[1] This review covers the period from 1931 to 1937. Previous reviews of the general field by the author appeared in this Journal, 1927, **24**, 473–487; 1929, **26**, 499–526; 1931, **28**, 505–532. In the present review, references to physiological studies have been largely omitted. The reader interested in this material should refer to the author's review in *Physiological Reviews*, 1937, entitled "Fatigue in Mental Work."

For example, it covers such problems as those of cumulative work decrement and transfer of work decrement.

From year to year, emphasis shifts. But in the last 6 years, the first of these problems has received the most attention, and if we can judge by the tenor of recent contributors, interest in the problem is growing. It is being attacked from several distinct angles, chief of which are: (1) a study of the muscular activities accompanying intense thought, and (2) a study of the effect of artificially induced muscular tension on the level of mental performance.

The first angle of attack is far from new. It goes back to the study of Lombard. But the advances have been made chiefly in the way of developing vastly improved techniques for determining what is occurring in the effector organs. Since the first necessarily crude methods employed by Morgan, *i.e.* response keys equipped with resistant springs, and by Jacobson in his first exploratory study, *i.e.* introspective reports of proprioceptive strain sensations, a gradual refinement of techniques has proceeded mainly along the line of more sensitive amplification until, at present, it is possible not only to measure extremely slight alterations of muscle tonus, but even to detect efferent innervations which are too slight to produce contractions, but which nevertheless may conceivably arouse proprioceptive impulses, and reinforce central processes. Of the methods in use, some are mechanical, some electrical.

The mechanical methods include measures of the extent of displacement of limbs, pressure exerted on a writing stylus (**41**), or on a bulb held in the palm of the non-active hand (**10, 11**), or the more sensitive method of muscle-thickening measures, introduced by Dodge. One variation of this, used by Freeman (**18**), is the muscle deformation method, whereby the movements of a bar pressed upon the tendon are amplified by a lever system many times and photographically recorded.

The main electrical method is to measure action potentials directly from the muscle by attaching electrodes and amplifying the currents through the use of an oscillograph or other instrument which will permit photographic recording (**26**).

It has usually been reported that some increase of tension in the muscles studied accompanies any augmentation in mental effort. This is the finding of Golla and Antonovitch (**23**), of Jacobson (**27**), of Freeman (**16, 17**), of Stroud (**41**), and of Clites (**8**). But Henley (**25**) reports quite variable results, sometimes obtaining an increase and sometimes a decrease with both psychotic and normal subjects when problems are presented to them. He also finds a

tendency toward sex differences and differences correlated with temperamental pattern. Perhaps the fact that he used a complex task, *i.e.* intelligence tests, and measured the tension only in the elbow by extent of movement, accounts for the discrepancy. At least it is significant that he always found some shift of tension. It is possible that sometimes there was a shift of already existing tension to other muscles in some of the subjects. Any theory of "effort" as some kind of motor set would lead one to expect a particular pattern of muscle tension to occur, and this might well differ from person to person.

However, important as these investigations undoubtedly are in demonstrating the intimate correlation of mental effort with increased muscular tonicity, they inevitably fall short of convincing the skeptic that the level of mental performance is affected by them, or that they play any more significant rôle than that of overflow phenomena, or incidental end-products of cerebral spread of excitation.

The crucial question is not whether mental effort is or is not accompanied by increased muscular tension, but whether or not it can occur without such increase, and whether or not induced tension has a dynamogenic effect on mental processes.

In the first experimental approach to the problem (3), an increase in muscular tension over the normal was induced by having the subjects squeeze dynamometers with both hands at a pre-determined pressure, while carrying out routine mental tasks, such as learning of nonsense syllables and words, adding columns of figures and reading off scrambled letters from large cards (a task analogous to color-naming). The results are somewhat ambiguous, since the amount and direction of the effects varies from subject to subject, from task to task, and from one criterion of efficiency to another. But the trend is toward an improved performance with increased tension, which may or may not lessen with practice, as the subject becomes more habituated.

These results have been corroborated in the main by later investigations, with two striking exceptions which will be discussed. An attempt has been made, in later studies, to control more adequately one or another of the factors incompletely controlled in the first study.

For example, it is not certain that, by increasing the muscular contraction in a given part of the body, one can be perfectly sure that the total amount of tension in the body has been increased. Perhaps, also, the significance of the change is qualitative rather than quantitative. Perhaps it is the particular pattern of contraction, the attitude or set, which is effective, rather than the absolute amount of increase in total bodily tension. This could be answered by taking

measures of the tension appearing in other muscle groups of the body simultaneously with its artificial induction in the given muscles. Freeman (**15**) did just this by taking records of quadriceps tonus during the time that his subjects were holding the "tension" set. This tension set was induced in 3 different ways, either verbally by responding to the command to "be tense," or by sustaining weights with the unused arm and hand, or by maintaining an erect sitting posture against gravitational pull. He found that these tension conditions always produced a spread of tonus to the quadriceps muscle in the leg, showing that quite remote muscle groups are actually affected. However there is still the possibility that the quadriceps tonus was an integral part of the pattern and that a compensatory relaxation occurred elsewhere. Certainly in any comparative study of the research of different investigators, we must make allowances for the different types of tension which were experimentally induced, and even more important is it to note the differences in posture which were assumed by the subjects in the normal or control condition. It appears to the reviewer that none of the studies, including his own, adequately standardized the control condition, so that this condition presented a clear contrast with the tension condition.

Block (**7**), for example, who clearly appreciated this need, and who made sure that there was a minimum of tension in the hands and feet during the control, by putting weak springs in the grips, nevertheless had her subjects seated in a straight back chair in a position in which they would get considerable trunk tension, and a position which they would not naturally assume for doing mental work. Perhaps one of the main reasons for the difference between the results of Block and of Zartman and Cason (**46**), on the one hand, and of Bills and Stauffacher (**6**) on the other, lies in the nature of the instructions for the control condition. The reviewer had the subjects rest their arms in their laps and lean back against the back of the chair, whereas, to grip the dynamometers, they were compelled to lean forward slightly. Stauffacher gave his problems in groups of 5 with plenty of time between for the subject to shift his set. Zartman and Cason shifted the set for every example. It is very doubtful whether a subject can shift his muscular set so rapidly without a carry-over. Perhaps the fairest kind of "control" instructions would be to tell the subject to assume the bodily attitude normally assumed for mental work.

Another question is that of degree of induced tension. One would hardly expect all degrees to be equally effective, and there is good reason to suppose that some degrees might be detrimental. This would be the natural inference from the results of Freeman (**15**) in an experiment designed to compare the effect of supra-maximal with that of maximal and sub-maximal tension. The first condition was

detrimental, as was the third. Two experimenters have made a special attempt to control this factor of degree of tension systematically. Block used 6 degrees, counting the control, as follows: control, 8 lbs., 15 lbs., 22 lbs., 48 lbs., and 56 lbs. The task was continuous addition. While no one degree was significantly superior for the group, nevertheless half the subjects found 15 and 22 lbs. superior when speed was the criterion, and two-thirds of them found it superior by the accuracy criterion. One would hardly expect all subjects to find the same optimum because of individual differences in strength, etc. For that reason, the method used by Stauffacher (40) appears to be somewhat better. He first obtained each subject's maximum possible capacity to pull against a dynamometer spring by gripping handles and pulling for one minute. He then divided this maximum into convenient fractions, *i.e.* one-half, one-fourth, and three-fourths. The maximum was not employed as an experimental condition, because it was obtained in a test pull of only one minute duration, and could not have been maintained long enough to complete some of the learning problems given. He found only one degree, that is, one-half maximum, which was significantly superior to the other degrees, and to the normal. This amount, of course, varied from subject to subject, within a range from 6 to 30 for men and 4 to 18 for women. Some of the difficulties encountered were that an initially beneficial degree of tension would cease to be so if held too long as it had to be in the case of slow workers, and that some subjects exerted themselves much harder than others in the preliminary test.

Freeman has suggested that the optimum degree of tension might vary considerably with the task. This certainly follows logically from his own results in the study in which he varied the strength of the incentive in order to vary the degree of motivation. If the incentive is too strong, the added discharge of nervous excitation into rival effector channels merely increases the competition between tendencies, resulting in blocking.

Again, there is the question of the degree of complexity of the mental task. Does tension affect complex and abstract intellectual operations in the same way as it affects simple routine ones? The early studies were limited to fairly simple tasks, as adding figures, reading letters, learning nonsense syllables, running mazes, reaction time to sounds, and manual pursuit. What of more ideational tasks such as solving arithmetical reasoning problems and intelligence tests? Zartman and Cason (46) chose arithmetical problems from a number of intelligence tests. Each subject solved 24 of these at a

sitting, alternating between tension and control conditions from example to example. If the subject took more than 3 minutes for an example, it was given up and an arbitrary score given. In a second sitting, 24 new problems were solved. Then, on days 3 and 4, they worked over again the problems of days 1 and 2, but changed the conditions, so that a problem originally solved under tension was now solved under control, and vice versa. Tension actually proved superior by both speed and accuracy criteria, on the last 2 days, but the differences were not statistically significant. The mean time under tension was 50.8 ± 1.6 seconds and under normal 54.8 ± 1.7 seconds, giving a difference of 4.0 ± 2.3 seconds. By the accuracy criterion, tension gave a mean error score of 23.6 ± 1.7, while normal gave 27.7 ± 2.2, a difference of 4.1 ± 2.8 or about 14 per cent improvement. Block (7) used analogies and syllogistic reasoning problems. Tension was induced in the hands, or the legs, or both together. Actually, none of the conditions gave significantly better average results than the control, but what positive trends did show up appeared in the more complex tasks, rather than in simple addition. Bills and Stauffacher (**6**) used 2 different complex tasks, arithmetical reasoning and solving of detectograms. The latter were short detective stories of 1 page each, in which the significant clue had to be gleaned from among a lot of irrelevant details. In the case of the arithmetical problems, the tension and control conditions were only changed every 5 problems, with half minute rests between, so that the set would not carry over. Twenty problems per day were solved for 4 days. Tension was secured by lifting and pulling weights of about 14 lbs. The results for arithmetical problems show a statistically significant advantage for tension in speed, but no difference in accuracy. In the case of the detectograms, the group averages gave negative results. But a new light was shed on the problem by dividing the subjects into 2 sub-groups, putting the 8 best performers into one and the 8 poorest into the other. It was immediately apparent that the poor subjects were benefited, while the good ones were detrimentally affected. This suggests the hypothesis that persons who habitually work efficiently and presumably near their capacity, are unable to benefit by the added spur of tenseness, but are distracted by it. On the other hand, persons who normally work inefficiently, or considerably below their maximum, need just the added tenseness to act as a constant spur. This needs a much clearer experimental verification.

A knotty question has been raised by those who argue that tension

may facilitate mental performance by acting as a distractor. The assumption is that if a distractor facilitates, it does so in a roundabout way, by creating an obstacle which the organism puts forth effort to overcome, either voluntary or unconscious, and in so doing overcompensates, thereby ultimately facilitating performance. This is supposed to differ from the direct reinforcement exerted by proprioceptive impulses, and to belong to a different category. Aside from the question of the legitimacy of calling a facilitator a distractor, which is discussed elsewhere in this article, it is very doubtful whether one can separate the total effect into any such distinct types. Even conceiving of the "overcoming of resistances" as a conscious adjustment, there is no evidence for such a process. Block, for example, reports that she found no correlation between her subjects' introspections as to the disagreeable or distracting nature of the induced muscular exertion and the objective effect on their performance. Many reported that they ceased to be aware of it.

Still another question is that of the relation between the efficacy of tension and the stage of practice in the task, as well as the amount of practice in working with induced tension. The evidence from studies of non-voluntary tension points to the presence of much more muscular involvement in the early stages of practice, with a tapering off toward the later trials. This seems quite logical, and could be explained in 2 ways: that facilitation is most needed in the early stages before performance becomes smooth and automatized, and that habituation lessens the excitatory strength of any persistent stimulus.

Studies which have emphasized this tendency are those of Freeman (15), Golla and Antonovitch (23), Henley (25), and Ghiselli (22). It is particularly manifest in Stroud's investigation of tension in stylus maze learning (41). An examination of his unpublished kymograph curves of stylus pressure shows high levels of pressure as a rule in the early trials, followed by a rapid tapering off in later ones, although the last trial usually shows an increase over the immediately preceding ones. This is doubtless due to the desire on the subject's part to fulfill the criterion of 3 perfect runs, thereby terminating the task. It is an end-spurt phenomenon.

In the studies of voluntarily induced tension, however, the relation to amount of practice is not so unambiguously shown. Bills (3) found, in the main, an increasing advantage of the tension condition from trial to trial, although this was reversed for recall and saving scores. But Block (7), on the other hand, gives comparative practice curves from trial to trial scores in addition, for

tension *versus* control, which tend toward convergence, though some actually diverge. A slight initial advantage for tension disappears or is even reversed by the fifth trial. Stauffacher found, in his study of tension in learning (**40**), that if he separated his subjects into a practiced and an unpracticed group, it was the unpracticed group that benefited most from the induced tension.

There is one more angle on the problem of muscular tensions which should be emphasized before we attempt to draw any broad general conclusions. It is suggested by a line of investigation which was begun by Buford Johnson and by Duffy (**10**). They were particularly interested in the relation of changes in muscle tension to emotional states. Duffy had her subjects, who were children, hold a rubber bulb in the unused hand while attempting certain tasks with the other. The tension changes which she got were apparently indices of emotional disturbance in the child and high tension children did not prove to be the most efficient performers. It may be that we have here a distinction between 2 quite different kinds of tension, one due to emotional upset, the other reflecting effort. The former type might well be inhibitive, because it indicates disrupted behavior. Hypertension has often been considered one of the most characteristic accompaniments of emotionality or nervousness. Luria developed his tremographic technique for detecting emotional reactions on the basis of a supposed correlation between left hand contractions and responses to critical words. It has long been known that emotionally toned stimuli elicit prolonged reaction times. This in itself is evidence of their inhibitive effect.

The distinction just made should be applied to the findings of Henley (**25**) with regard to the tensions developing in psychotic patients when presented with intellectual problems. He found that manic manics were habitually more tense in the resting state, when not engaged in the task, than any of the other groups, particularly the schizophrenic group. This sort of tension could be attributed to their generally excited state. But these same manic manics showed a drop in tension as soon as they were given the mental task. The schizophrenics, on the contrary, whose resting or pre-task tension was low, showed an increase when confronted with the task. This was evidence of the assumption of a task set, and probably bore a closer relation to "effort" than the other type. Incidentally, Henley disproved the theory that all psychotics and neurotics are characterized by hypertension, or variable tension. The men were less extreme than normal men.

What general inferences, if any, can be drawn from all these studies of the relation of muscular tensions to mental effort? Does the evidence justify the conclusion that effort *is* essentially muscular in its mechanics, and that mental effort is not essentially different in this respect from other forms? Physical effort, that is, intense effort, brings into play many remote muscle groups, which, while not directly involved in carrying out the act, are nevertheless necessary reinforcers. These have been called "remote controls" as opposed to "resident controls" in such a task as ergographic work. Or must we retreat to the position, suggested by Lashley, that any concomitant motor activities accompanying mental effort should be considered nothing more than overflow phenomena, end products of spread of excitation without functional significance in the act of thinking?

The reviewer believes that the evidence is still insufficient to decide, and that what is needed is an entirely new angle of attack on the problem. Much encouragement can be derived from the rapid advances in techniques for measuring changes in muscle tonus. Much progress should result from the better understanding of the sources of error in earlier work, and the invention of better controls to eliminate them. The criticism of Block (7), that the volitional and motivational factors involved in experiments in voluntarily induced tension invalidate this line of approach, is a legitimate one. The seriousness of it is increased when we consider that it is probably the particular *pattern* of tension rather than the mere quantitative amount which is significant. Is there a particular "effortful" bodily attitude, which, when assumed, induces greater mental alertness? Or is this a will-o'-the-wisp, like the supposed characteristic facial expressions of different emotions, which Landis was unable to substantiate? And, in asking a subject to assume such an attitude, are we not imparting to him a subtle suggestion?

The new angle of attack might be along the line of Harlow's study of learning in curarized animals. By the use of drugs, it might be possible to systematically remove tensions from every different skeletal muscle group, at different times, to determine whether effortful thought is interfered with by the loss of any particular set of tensions. When the subject is himself asked to relax different muscles, as in Jacobson's studies, a suggestion may be imparted. More work should be done on the effect of muscular exertion on specific functions, such as sensory thresholds, control of images, reaction time. It should be used in states of lowered functioning

such as are produced by loss of several nights of sleep, on first arising in the morning, when drowsiness has been induced by atmospheric conditions, or in extreme fatigue. Freeman (14) has already paved the way with his study of compensatory reinforcements subsequent to sleep loss.

Before dismissing the problem of the mechanics of effort, the point should be stressed that there are indubitably other ways in which mental operations are reinforced beside the roundabout way of proprioceptive stimulations. Recent neurological evidence points to the possibility of direct interplay of central excitations, and the principles of convergence and final common path can be assumed to operate from centrally as well as peripherally initiated impulses. One can never be certain, therefore, which produces whatever effects are obtained, provided this tension results from central impulses. Tension, therefore, should be induced mechanically in some manner by direct stimulation of the muscles electrically, and the effect upon ongoing mental processes noted. Both quantitative and qualitative variations in tensions could be elicited, independently of any central determination. The results should throw light on the question of the rôle of proprioceptive impulses, thereby narrowing the problem within experimentally explorable limits.

The second major problem is that of the *dynamics* of effort. It includes incentives and motives. Motives are conative attitudes of the organism which predispose it to respond energetically with reference to particular stimuli. They may or may not be conscious, and they may or may not be intrinsic to the task, but the satisfaction of them constitutes a directive factor in behavior. The stimulus objects which satisfy motives are called incentives. Thus, food is the incentive which satisfies the hunger motive, and praise is the incentive which satisfies the social approval motive. These should not be confused, because it is wrong to assume that the variation of any given incentive guarantees that the corresponding motive is being varied comparably or that it is even present.

Experimental studies of motivation have been mainly directed toward such problems as that of measuring the strength of an incentive; measuring the relative strength of different motives as shown by the vigor of response to the appropriate incentives; the effect of varying the objective strength of the incentives, singly or in combination; the shifting of incentives; the relative effectiveness of positive and negative incentives; and the rôle of social motives. The 2 most important contributions of the last few years to a dis-

cussion of experimental literature on this subject are *Motivation of Behavior* by P. T. Young (45), and *The Experimental Psychology of Motivation* by Diserens and Vaughn (9). These authors have covered the literature exhaustively and few important additional studies have appeared since. Only part of these authors' discussions, however, has significance for the problem of facilitation and inhibition. Some students of mental work have considered that the problem of motives is inseparable from that of variations in the strength of the work stimuli responded to. Thus working under a weaker incentive means simply responding to a work stimulus of altered strength. There are certain differences, however. The incentives used in work are often extrinsic to the main work set or *aufgabe*. In this respect, they are not mere variations in the strength of the work stimuli themselves. On the other hand, they appeal to actual conative attitudes of the organism and, hence, are distinct from the extraneous stimuli like distractors, etc., which merely act mechanically according to the principle of summation or interference of peripheral excitations. The latter group constitute the problem of the next topic.

The recent experimental papers on motivation center around 2 problems: (1) the effect of competition on performance level, and (2) the effect of the subject's appraisal of his ability in relation to the task. This appraisal may be the result of suggestions from the experimenter or the result of the subject's own knowledge of his past performance. Suggestions from the experimenter may take the form of estimates of the difficulty of the task, or estimates of the subject's performance. Manzer (30) tested the maximum hand contractions of a group of men and women on a Smedley dynamometer, under suggestion that the task would be easy, or medium or difficult, even though it was always objectively the same. With men, the first 2 suggestions caused a reduction of 7.25% and 4.82% respectively below the work done following the control suggestion. The suggestion of "difficult" on the contrary increased output 4.66% over the control and 11.35% over the suggestion "easy." With women "easy" and "medium" caused no reduction, but "hard" caused a 7% increase. Variability was greater for all experimental conditions.

This is a pure muscular task. Would the same effect appear in mental tasks? McKinney (33) gave subjects instructions in a maze learning task, a multiplication task and a motor coördination task, calculated to arouse feelings of inferiority. They were told that anyone of average intelligence could complete the task in a period

which was actually fictitiously short. He then sounded an alarm at the end of this period to warn them of the passage of time. He thereby set up the expectation that the task was easy, but also very definitely appealed to what Frank (12) has called the ego-level, *i.e.* the level which the subject wants to maintain in the experimenter's eyes. McKinney found wide individual differences in the susceptibility of his subjects to this suggestion, but it invariably disrupted their performance to some extent as compared to a control group and as compared to a differently motivated group. The remarks of the subjects indicated a great amount of emotional conflict.

Another sort of suggestion operated in the study by Vaughn (43) which definitely affected his subjects' performance. The task consisted in shooting at a target. The competitive conditions were either (1) "high score," (2) "handicap" or (3) "improvement," depending on whether they shot to get highest score, or to equal their handicap against an expert marksman, or to show the greatest improvement over their past scores. The results were a function of the ability of the subject. The good performers excelled under the "high score" and "handicap" conditions, while the poor performers preferred the "improvement" condition. It is concluded that one's opinion concerning the possibility of his success is important in determining the direction in which his greatest energy will be expended.

A number of studies have indicated that a knowledge of one's past performance or of one's output at the moment, is a strong facilitator. Two recent confirmations of this which need no further discussion are those of Manzer (31) and Ross (38).

A study by Frank (12) introduces a new angle. The question raised is not concerning the effect of knowledge of output on the task at hand, but its effect on the level of aspiration in a subsequent task. It is definitely concerned with the transfer of work set. Frank found that the subsequent level was closely affected by the previous level, but he also noted exceptions which could only be accounted for by assuming that the subject's ego-ideal entered in as a determinant. That is, his realization of a poor level of performance tended to make him exert extra effort in the later task to regain his self-esteem.

Our third problem is concerned with the effects of extraneous stimuli on mental performance. These extraneous stimuli may or may not be incorporated into the ongoing activity in such a way that they eventually form part of the task set. The essential point is that, at the outset, they are irrelevant to it. The assumption was originally made that such stimuli must necessarily detract from the attention given to the task, and must therefore exert a disruptive

effect. For this reason they were called distractions. An imposing number of experimenters found, however, that the effect was just as likely to be facilitative. The term "distraction" no longer had a precise meaning. From that time on, the problem has been to discover what characteristics of extraneous stimuli determine whether they shall exert a facilitative or an inhibitive effect.

Certain factors have already been isolated. First, is the matter of timing. If the extraneous stimulus occurs at a given time interval before the task stimulus, it exerts a reinforcing effect. If it occurs at other time intervals, it inhibits the act. This is illustrated in the knee jerk experiment. Now when, instead of a discrete stimulus and a discrete act, there is a continuous series of stimuli and a continuous mental performance taking place, the temporal picture is so complicated that it is difficult to predict what will occur.

Second, is the question of relative strength of the task stimulus and the extraneous stimulus. The more favorable situation for facilitation is that in which the task stimulus is the stronger.

Third, is the question of how easily the extraneous stimulus can be incorporated into the task set, so that it bears some rational relation to it. For example, auditory stimuli delivered at regular intervals can become incorporated into the task as pacers, provided the intervals are somewhere within the natural work rhythm of the subject. Otherwise, such stimuli can be decidedly disruptive. For example, the regular beat of a metronome has been found to alter the rate of work within wide limits. Fryer concludes from one study that auditory stimulation, such as noise or rhythm, does not influence accomplishment in mental work, unless consciously accepted as an incentive. But this is so contradictory to the findings of previous investigators, notably Morgan and Ford, that it warrants careful examination.

In his first study, Fryer (20) used addition tests as the task and a pattern of 5 bell tones of different pitches played at a regular rate of 2 seconds per gong, as the auditory stimulation. Written reports were called for after each test of 8 minutes so as to detect any changes in the subject's motivation. The changes in stimulation used were (a) reversing the order of pitches of the tones, (b) increasing the interval between bells, (c) playing one gong rapidly, and (d) eliminating all auditory stimulation. The subjects were instructed to work as rapidly as possible. But actually, each subject developed a different "intent" or work set, as shown in their introspections. They also developed different "intents" with regard to

the bells. It is Fryer's contention that the objective effects on speed show a close correlation with the sort of "intent" which each individual subject develops toward the bells or toward the change in the character of the auditory stimulations. This may result in either facilitation or inhibition, but whichever it is, it is determined by the nature of the subject's conscious intent. This conclusion practically necessitates explaining all effects of extraneous stimuli on a conscious basis. It seems to the reviewer that the set up of the experiment was such as to encourage the subjects to react to the bells on a conscious basis. And the results of only 4 subjects lack conclusiveness for the problem, inasmuch as it is so easy to rationalize as to the relation between the direction of the behavior change and the intent. On the other hand, other investigators made no systematic attempt to determine the subjects' "intent."

In a second experiment, Fryer (**21**) held pitch and loudness of the bell stimuli constant and varied only rate, making this similar to the subject's work rate, or faster or slower. Again he found that the objective auditory timing failed to influence the rate of the repetitive work unless it first changed the worker's conscious intent. The usual result was for the subject to ignore the external rhythm and hence be uninfluenced by it unless it approximated his work rhythm.

It is an entirely different matter when the rhythm or pace setting is an intrinsic part of the task set. Two studies, in which increasing speed of stimulation was used in a mental task, give results which are partly in agreement and partly at variance.

Taylor (**42**) used a highly complex task, involving discrimination between 5 light intensities, between 3 auditory stimuli, and between 2 peripherally exposed lights, by responding differentially to each in a specific manner. The stimuli were presented in a random order. In the normal phase (A) each was presented for 1.5 second with intervals of 1.5 second between. In phase (B) no time was allowed between stimuli; in phase (C) they halfway overlapped each other; phase (D) was identical with the normal.

The result of the speeding up was a decrease in accuracy, and an increase in variability and incoördination. When the normal rate was resumed, the decreased efficiency carried over. The obvious conclusion would seem to be that any increase in the externally imposed rate of work results in loss of efficiency. But this conclusion is unjustified, for several reasons. First, this task is extremely complex; second, the original speed was near the maximum for such a task; third, the increases in rate were very large and abrupt to 2 then 3 times normal; and fourth, the overlapping of stimuli in

phase (C) was calculated to cause anticipation and hence confusion, regardless of rate.

Bills and Shapin (4) investigated 2 problems: (1) the relation between rate of work, as automatically controlled by rate of presentation of stimuli, and efficiency, and (2) a comparison of efficiency under subject-controlled rates of work *versus* that under controlled rates. The work was color naming and form naming. The speeds varied from 40 stimuli per minute to 120 per minute. Efficiency was measured by computing errors per minute, blocks per minute, and block length. The results show that there is an optimum rate of work at which efficiency is at a maximum, and this varies tremendously from subject to subject. Any increase or decrease in the externally controlled rhythm is detrimental by all criteria. The second problem was studied by the use of an apparatus for presenting the stimuli which could either be controlled by the subject at his own rate or by an automatic timer at any given rate. It was found that when working at the "paced" rate the subjects showed a smaller increase in frequency and length of blocks than when working at an equivalent "unpaced" rate. In fact, the average subject working at a paced rate of 83 r.p.m. had no more blocks than he did at a self-determined rate of 63 r.p.m. These results are not necessarily at variance with Taylor's because he did not use a subject-determined rate in his study, but they show that externally imposed rhythms and work rates do exert a dynamogenic effect provided they are accepted as intrinsic to the task.

What is the effect of extraneous stimuli which are completely random, with no temporal pattern, and incapable of being incorporated into the work set? Such were the conditions set by Harmon (24), who studied the effect of noise on output and metabolism.

The noises were made by records taken from a busy office and a noisy street corner. Performance was measured by amount accomplished and accuracy. Only 2 subjects were used, unfortunately. In agreement with previous investigators, he found that the introduction of the noise created an initial retardation in performance, which showed up most in accuracy, but that this initial retardation was overcome and even reversed as the subjects became somewhat accustomed to the noise. The increased metabolic rate, however, showed that any advantage in performance was matched by an increased energy cost.

Laird (29) experimented with variable and steady complex noises, and pure tones of different pitches, as they affected a task

involving complex eye-hand coördination. The noises varied in intensity from 48 to 90 decibels. He found that the varying complex noise had the most disruptive effect on production, the steady complex noise next, and the pure tone least. The effect of the pure tone depended on its pitch. A pitch of 512 cycles was worse than any lower pitch, and every increase above this was followed by a loss in output. Any reduction in the complex noises was accompanied by increased output. He reports no such compensatory improvement, or adaptation to the noise as Harmon and others have reported, and this in spite of the fact that the work period was 4½ hours long, and, in some cases, the records given were from the latter hours only. The noise very definitely increased the rate at which fatigue developed during the work period.

It is very difficult to reconcile such definite and consistent findings as these, in which extraneous stimuli are invariably inhibitive in their action, with the frequently quoted results of Morgan and the large number of other experimenters who obtained facilitative effects at least part of the time. Was there some difference in the subjective attitude taken toward the noise? Harmon reports that the attitudes which his subjects expressed toward the noise had no relation to the actual objective effects shown in their performance.

The only valid conclusion which seems justifiable is that the factors involved in such studies are so complex and variable as to preclude any consistency of results from different experimenters. No general rule can be formulated about the effect of extraneous stimuli, and certainly no prediction can be made as to what would be the outcome of any particular set of conditions.

Our fourth major topic is concerned with the inhibitive or facilitative effects of mental work on itself. It includes both the effect of previous on subsequent performance, either in the same or a different mental task, and interaction of tasks carried on simultaneously. An important recent discussion of this material is that of E. S. Robinson, in the *Handbook of General Experimental Psychology* (**35**).

The question of the course of the general decrement in the work curve for mental *versus* muscular work has interested students in the past, because apparently the curves obtained from the 2 different forms of work were quite different. The ergograph curve (obtained from pulling weights) was supposed typically to be positively accelerated, or in other words to maintain a fairly steady high level of excursion per stroke until near the end and then to decline rather

rapidly until a complete breakdown was reached. The mental work curve, on the contrary, typically showed a negative acceleration, *i.e.* a rapid falling off during the early stages (when warming up was absent) and then gradually leveling off, so that it approached the horizontal. These differences were thought to be fundamental. But recent studies, in which spring dynamometers were used in producing the ergograms, have proved that the typical curve for this type of muscular performance is in fact negatively accelerated like the mental work curve.

Marks (**32**) quite consistently obtained such concave shapes, and Yochelson (**35**, p. 581) reports some tendency in that direction even in the case of weight pulling curves. It is evident that the difference is not as fundamental as was supposed, and that there is enough of the element of volitional effort involved in ergographic performance to allow the same typical phenomena of central control to show themselves.

Another striking thing to note in the ergographic curves obtained by Marks and by Yochelson is the fact that each subject's work curve tends to have typical features which are characteristic of him, and which may differ considerably from those of other subjects. Marks made this the basis for an investigation of personality differences. He also tested the hypothesis that the character of the curve is related to some fundamental difference in personality, so that the ergogram might be used as a test of personality type. From each of 164 subjects he obtained 2 spring-ergograph records, and classified them as to work rate, change in work rate, contraction height, change in contraction height, irregularity, and tension after pulls. The correlations between measures ranged from .55 to .79, but the shapes were not consistent. No reliable "type" differences resulted, but one sort of group difference that did emerge was the tendency for certain subjects to spontaneously assume a high speed attitude, which disturbed the homogeneity of the work rate distribution.

His indifferent results regarding the relation of ergogram type to any such type system as that of Kretchmer are somewhat contradictory to the promise held forth in certain previous published studies (**28**, **39**), but are more in keeping with modern attitudes toward the whole question of the existence of types.

New evidence has accumulated with reference to 3 of the "principles of the work decrement" formulated by Robinson (**35**). Principle 4, in his list, states: the work decrement of a given S-R connection is relative to the strength of that specific connection.

Obviously the strength of such connections depends on the degree to which they have been exercised. A practiced reaction should therefore show a less rapid decrement from continuous use than an unpracticed one. However, a certain amount of practice gain develops within each working period, and during the early stages of practice in a task, enough of this gain might accumulate during a single work period to completely mask the decrement which would otherwise manifest itself. And, conversely, in later work periods, less of this practice gain would accumulate within the single period, so that the decrement would show up more clearly. In this way, the law formulated above would be concealed, and it would superficially appear that decrements were greater in the later work periods. A time would come finally, however, when practice would have virtually ceased, and the full decrement would stand out. This is what actually happened in the study of Robinson and Robinson (**36**), whose 10 subjects wrote letter sequences 20 minutes per day, 5 days a week for 3 weeks. With the exception of the first day, the greatest decrement was in the early days of the experiment.

Principle 2 of Robinson's list states: the work decrement of a given S-R connection is relative to the frequency of previous functioning of that connection. This is usually assumed to mean that continuous work causes a slowing up in speed and an increase in errors, the 2 commonest criteria of efficiency. But such decrements do not invariably occur, even when the task fulfills the requirement of being highly homogeneous. And when they do occur, they are often relatively insignificant in amount. Does this mean that there is no cumulative refractoriness? The answer is that instead of a gradual increase in reaction times, there is a recurrence of prolonged reaction times, which appear, on the average, about 3 times a minute and are more than double the modal reaction time, but increase in frequency and length as work continues. It has been suggested that these blocks are phenomena of refraction. When the modal reaction time is computed, with the blocks omitted, it is found not to increase at all with continuous work. The apparent slowing up in reaction time, therefore, is an artifact, resulting from the increase in blocks. The blocking is subject to the laws of homogeneity and competition (**2**). That is to say, the fewer elements there are involved in the task, the more rapidly the blocking increases, unless the various response tendencies tend to conflict. In the latter case, the greater the number of elements involved in the act, the greater the rate of increase in blocking. Such a task as color naming

gives a more rapid increase in blocks, the greater the number of different color stimuli used.

Any increase in the rate of work causes an increase in the frequency and length of blocks, until, with extremely rapid rates of presenting the stimuli, blocking is practically continuous, and what responses do occur are errors, indicating that they are purely random in all probability (4).

The first work on blocking seemed to point to an approach toward regularity or periodicity in its occurrence, and suggested an interpretation in terms of some fundamental organic rhythm; but as more data accumulate, the decided irregularities become more apparent, and cast doubt on the existence of any exact rhythm, either as to time, or as to number of units of work done (*i.e.* amount of energy expenditure). The results obtained seem to be a composite effect involving the contribution of recurrent refractory states of more than one fundamental mechanism of response, plus disturbances due to adventitious factors. Some evidence seems to point to at least 2 such factors, one much more frequent in occurrence than the other (1).

A different sort of phenomenon, but perhaps related in some way, has been studied exhaustively from a mathematical angle by Philpots (34). We refer to his analysis of fluctuations of output in the work curve. He rejects the common assumption that such periodicities bear an arithmetical ratio to one another and suggests instead a geometrical progression. This means that the successive recurrent fluctuations occur at ever widening distances from one another as work continues. Beyond an hour, this would lead to such long waves that it would not be feasible to study them. Philpots concludes that any given work curve is a composite effect of a number of different component periodicities which must be analysed from the complex wave much as tones are partialed out from complex sound waves. He offers no answer to what is the organic basis for these many different periodicities, except to suggest that they relate to basic nervous factors.

That periodicities are present even in finger-ergographic work, is the conclusion of Freeman and Wonderlic (19), who made a preliminary study of fluctuations in the length of the excursions of the finger when rate was held constant, and in the number of excursions per minute when extent was arbitrarily limited. Their method of studying periodicity was to compute the output in each five-second interval of the twenty-minute work period, and average these and

obtain the average deviations. Those intervals whose output deviated from the average by one or more A.D. were considered marked fluctuations. Frequency tables constructed for each of the 40 subjects showed that regular periodicities in these fluctuations occurred for 21 of the 40 subjects, but only 14 subjects had the same modal interval in both tests. The distributions were either unimodal or bimodal. The other 19 subjects showed multimodal distributions. In general the subjects who showed the greatest variability in work output gave the least evidence of periodicity.

We are tempted to suggest that the exact mathematical approach to these phenomena will not yield much of significance because so many extraneous matters enter in to mask the results even under the most rigidly controlled conditions. But this does not in the least lessen the importance of the phenomena in the study of mental performance. The older criteria of efficiency, speed and accuracy, which have always been used as indices of level of performance, are not sensitive enough to be used in studies of the influence of various physiological and environmental agents on behavior. But the author has found the blocking phenomenon, at least, to be a more sensitive variable, which correlates more closely with the action of such agents. If the straining for exact mathematical results is given up, and a grosser statistical treatment more adapted to the phenomena is substituted, then a study of output fluctuations, and particularly of blocks will probably yield significant returns. This is already evident from preliminary studies of the relation of blocking to sleep loss, to anoxemia, to diurnal and temperature variations, etc., either completed or in progress.

Returning to Robinson's principles of the work decrement, Principle 7 states that: the decrement in a given S-R connection is relative to the decrements which have developed in other S-R connections. This is the principle of transfer of fatigue. It does not specify between what S-R connections such transfer of decrement is most likely to occur, nor whether it may be expected to occur between any and all connections, or only those which are related to the given connection in some significant manner. If these questions could be given a clear experimental answer, they would throw as much light on the nature of mental organization as studies of transfer in learning. In fact, the same principles which explain transfer in learning should explain fatigue transfer, for fatigue decrement is presumably due to the weakening of the very connections whose strengthening is brought about by the learning. One possible

explanation would be in terms of the common elements theory. If 2 different tasks involve common elements, then the transfer of decrement between them should be proportional to the number of such elements. A preliminary experimental confirmation of this hypothesis has been made (5).

Another explanation might be given in terms of the concept of transfer of sets or work attitudes. That is, a given speed set acquired in one task has been shown to alter the performance in a subsequent task (12) regardless of the subject's actual capacity to perform the second task at the time. Now if a mental attitude of disgust or boredom should be acquired in one mental task, it might have enough generality to carry over to almost any subsequent mental task. Certain muscular attitudes might conceivably carry over also. Most mental work is done in a rather cramped posture, and if continued for long necessitates the suppression of those strong urges to motor expression which are normal in active persons. Such postural attitudes as have to be maintained must necessarily continue over into any subsequent mental task. Evidence has, in fact, been reported that the performance level in a given task is beneficially affected by a mere shift of bodily posture.

That change of work eliminates part of the work decrement and is therefore superior to continuous work on a single task, is the theme of an experiment by Vickery (44), which confirms Chapman's earlier study. Eighty subjects were divided into 4 equated groups. One group worked at cancellation continuously for 20 minutes; a second group performed an equation test for a like period. The other 2 groups alternated work every 5 minutes on the 2 tasks, one beginning with cancellation, the other with the equations. She found, as had Chapman years before, that the change-of-work conditions were superior to the continuous-work condition, but she also found, as he did, that this was much more evident in passing from addition to cancellation than vice versa. Since continuous cancellation produced no decrement, whereas the continuous equation test did, the latter must have been the more difficult, and a change from it to an easier task might be expected to be correspondingly more restful. The results would be much easier to interpret if both tasks had been equally difficult, and if the difference in character between the 2 tasks could have been formulated more precisely. As it is, we can only conclude that they differed mainly in difficulty, and that a change from harder to easier was restful.

The thing most needed at present, if significant results are to be obtained from studies of transfer of fatigue, is a number of hypotheses regarding significant ways in which mental mechanisms may be related and interact. Then there should be a systematic classification of mental-work tasks in accordance with the quantitative degree to which they do or do not involve such relations.

BIBLIOGRAPHY

1. Bills, A. G., Fatigue, Oscillation and Blocks. *J. Exper. Psychol.*, 1935, **18**, 562–573.
2. Bills, A. G., Some Causal Factors in Mental Blocking. *J. Exper. Psychol.*, 1935, **18**, 172–185.
3. Bills, A. G., The Influence of Muscular Tension on the Efficiency of Mental Work. *Amer. J. Psychol.*, 1927, **38**, 227–251.
4. Bills, A. G., and Shapin, M. J., Mental Fatigue Under Automatically Controlled Rates of Work. *J. Gener. Psychol.*, 1936, **15**, 335–347.
5. Bills, A. G., and McTeer, W., Transfer of Fatigue and Identical Elements. *J. Exper. Psychol.*, 1932, **15**, 23–36.
6. Bills, A. G., and Stauffacher, J. C., The Influence of Voluntarily Induced Tension on Rational Problem Solving. *J. of Psychol.*, 1937. (In Press.)
7. Block, Helen, The Influence of Muscular Exertion upon Mental Performance. *Arch. of Psychol.*, 1936, **28**, No. 202. Pp. 49.
8. Clites, M. S., Certain Somatic Activities in Relation to Successful and Unsuccessful Problem Solving. Part 3, *J. Exper. Psychol.*, 1936, **19**, 172–192; Part 2, 106–115; Part I, 1935, **18**, 708–728.
9. Diserens, C., and Vaughn, J., The Experimental Psychology of Motivation. *Psychol. Bull.*, 1931, **28**, 15–65.
10. Duffy, E., Relation Between Muscular Tension and Quality of Performance. *Amer. J. Psychol.*, 1932, **44**, 535.
11. Duffy, E., The Measurement of Muscular Tension as a Technique for the Study of Emotional Tendencies. *Amer. J. Psychol.*, 1932, **44**, 146–162.
12. Frank, J. D., The Influence of the Level of Performance in One Task on the Level of Aspiration in Another. *J. Exper. Psychol.*, 1935, **18**, 159–171.
13. Freeman, G. L., Changes in Tonus During Completed and Interrupted Mental Work. *J. Gener. Psychol.*, 1930, **4**, 309–334.
14. Freeman, G. L., Compensatory Reinforcements of Muscular Tension Subject to Sleep Loss. *J. Exper. Psychol.*, 1932, **15**, 267–283.
15. Freeman, G. L., Facilitative and Inhibitory Effects of Muscular Tension on Performance. *Amer. J. Psychol.*, 1933, **45**, 17–52.
16. Freeman, G. L., Mental Activity and the Muscular Processes. *Psychol. Rev.*, 1931, **38**, 428–449.
17. Freeman, G. L., Spread of Neuromuscular Activity During Mental Work. *J. Gener. Psychol.*, 1931, **5**, 479–494.

18. FREEMAN, G. L., The Measurement of Tonus by Deformation of the Tendon. *Amer. J. Psychol.*, 1930, **42**, 581–590.
19. FREEMAN, G. L., and WONDERLIC, E. F., Periodicity in Performance. *Amer. J. Psychol.*, 1935, **47**, 149–151.
20. FRYER, D., Motivation Effects of Auditory Timing upon Repetitive Mental Work. *Brit. J. Psychol.*, 1934, **25**, 140–169.
21. FRYER, D., A Genetic Study of Motivation under Changing Auditory Situations. *Brit. J. Psychol.*, 1934, **24**, 408–433.
22. GHISELLI, E., Changes in Neuromuscular Tension Accompanying the Performance of a Learning Problem Involving Constant Choice Time. *J. Exper. Psychol.*, 1936, **19**, 91–98.
23. GOLLA, F. L., and ANTONOVITCH, S., Relation of Muscle Tonus and Patellar Reflex to Mental Work. *J. Ment. Sci.*, 1929, **75**, 234.
24. HARMON, F. L., Effects of Noise on Certain Physiological and Psychological Processes. *Arch. of Psychol.*, 1933, **23**, No. 147. Pp. 81.
25. HENLEY, E. H., Factors Related to Muscular Tension. *Arch. of Psychol.*, 1935, **27**, No. 183. Pp. 44.
26. JACOBSON, E., Electrical Measurements of Neuro-muscular States During Mental Activities. *Amer. J. Physiol.*, 1930, **91**, 567–608; **94**, 22–34; **95**, 694–702 and 703–712; 1931, **97**, 200–209.
27. JACOBSON, E., Electrophysiology of Mental Activities. *Amer. J. Psychol.*, 1932, **44**, 677–694.
28. KORNGOLD, S., Contributions a l'étude de la constance des sujets dans les efforts moteurs. *Le Travail Humain*, 1933, **1**, 186–191.
29. LAIRD, D. A., Influence of Noise on Production and Fatigue as Related to Pitch, Sensation Level, and Steadiness of the Noise. *J. Appl. Psychol.*, 1933, **17**, 320–330.
30. MANZER, C. W., Effect of Verbal Suggestion on Output and Variability of Work. *Psychol. Clin.*, 1934, **22**, 248–256.
31. MANZER, C. W., The Effect of Knowledge of Output on Muscular Work. *J. Exper. Psychol.*, 1935, **18**, 80–90.
32. MARKS, E. S., Individual Differences in Work Curves. *Arch. of Psychol.*, 1935, **27**, No. 186. Pp. 60.
33. MCKINNEY, F., Certain Emotional Factors in Learning and Efficiency. *J. Gener. Psychol.*, 1933, **9**, 101–115.
34. PHILPOTS, S. J., A Theoretical Curve of Fluctuations of Attention. *Brit. J. Psychol.*, 1934–1935, **25**, 221–255.
35. ROBINSON, E. S., Work of the Integrated Organism. Pp. 571–650 in Murchison, C. (Ed.), *Handbook of General Experimental Psychology*. Worcester: Clark Univ. Press, 1934.
36. ROBINSON, E. S., and ROBINSON, F. R., Practice and the Work Decrement. *Amer. J. Psychol.*, 1932, **44**, 547–551.
37. ROBINSON, M. F., Is the Refractory Phase Theory Adequate to Explain Mental Fatigue? *Psychol. Rev.*, 1931, **38**, 229–241.
38. ROSS, C. C., The Influence on Achievement of a Knowledge of Progress. *J. Educ. Psychol.*, 1933, **24**, 609–619.
39. SKANNON, P., Die Typologie der Ergogramme und ihre Beziehung zu Energietypen und zu den Körperbantypen Kretchmers. *Psychotechn. Zh.*, 1931, **6**, 1–19; 49–63.

40. STAUFFACHER, J. C., The Effects of Varying Amounts of Induced Muscular Tension on the Learning Process. *J. Exper. Psychol.*, 1937. (In Press.)
41. STROUD, J. B., The Rôle of Muscular Tensions in Stylus Maze Learning. *J. Exper. Psychol.*, 1931, **14**, 606–631.
42. TAYLOR, J. H., The Effect of Increasing Speed of Stimulation on an Organized Task. *J. Gener. Psychol.*, 1935, **12**, 132–143.
43. VAUGHN, J., An Experimental Study of Competition. *J. Appl. Psychol.*, 1936, **20**, 11–15.
44. VICKERY, K., The Effects of Change of Work on the Work Decrement. *J. Exper. Psychol.*, 1931, **14**, 218–241.
45. YOUNG, P. T., *Motivation of Behavior.* New York: John Wiley & Sons, Inc., 1936. Pp. xviii+562.
46. ZARTMAN, E. N., and CASON, H., The Influence of an Increase in Muscular Tension on Mental Efficiency. *J. Exper. Psychol.*, 1934, **17**, 671–679.

BOOK REVIEWS

SARTON, GEORGE, *The Study of the History of Science*. Cambridge: Harvard University Press, 1936. Pp. 75.

To say that scarcely a page of this little volume fails to stimulate a sharp difference of opinion, far from aspersing the value of Mr. Sarton's generalizations, only serves to heighten it. Inaugural lectures have commonly had the dual character of stimulating controversy and of pointing a method; in both respects the interest of a wide audience is aroused. Mr. Sarton's lecture provides no exception here, for many of his remarks water not only the fields which he cultivates but also the neighboring acres in history and in science.* His very approach places his topic on such a plane that all interested in the aims and methodology of learning can appreciate the value of his observations and suggestions. Despite the current and not always intelligent prejudice against "Education" in many university circles, it is safe to emphasize that a sound method of approach stands equal with a sound objective. In fact the two should not be treated as separate: they are but the two sides of the same coin. Perhaps we might go a little farther and give prior emphasis to method because it is the one aspect of a problem that can draw a meaningful discussion from a diverse group of savants. *La méthode, c'est l'homme.* In view of the fact that scientists have steadily been, and historians, especially those occupied more exclusively with social and intellectual phenomena, are increasingly concerned with the development of a method that will give meaning to their work, this lecture achieves genuine significance. Since, when the facts speak for themselves, they give not one answer but a myriad, method achieves greater importance.

Beginning with the suggestion that inaugural lectures are valuable as milestones in the career of a given discipline, Sarton emphasizes that the history of science is a relatively new discipline, as yet unmastered and lacking a single book truly worthy of the subject. Because the history of science—the history of the acquisition and

* Reference should be made to the bibliography of some twenty pages, containing most valuable suggestions concerning scientific journals, books, and societies, their history and character.

systematization of skills and techniques—alone can illustrate the progress of mankind, we must, he insists, focus our attention on this branch of knowledge. However, this does not necessitate disregarding the realms of function and value where admittedly progress cannot be measured—indeed does not seem even to exist.

But how shall the history of science be approached? The historian knows no science, the scientist no history; the first cannot speak with intelligent precision of any scientific problem and the latter has not an iota of perspective by which to give meaning to that same problem. When he attacks this problem Sarton speaks much more as a scientist than as a historian, and he does not seem quite sure as to whether he wishes to place history in the same category with chemistry, for example, or to treat it as an entirely different sort of discipline. Making a distinction between the preparation for a scientific and an historical career, he emphasizes that the preliminaries in the first instance are much longer and more systematic. He displays not a little intellectual naïveté when in referring to his examination of the work of an eighteenth century Transylvanian chemist he states that, knowing chemistry, he could far more easily acquire a knowledge of the historical *milieu* than a historian could remedy his deficiencies in chemistry. To which it may be said that perhaps an *adequate* knowledge—a knowledge that would satisfy an exacting and penetrating historian—would be as hard to acquire as the chemical knowledge which Mr. Sarton, a scientist, would demand of the historian. Too often the historical aspect of a given problem has been looked at as something that could be worked up on short notice, and it is forgotten that the item worked up in that way may bear the stamp of its workmanship no less than any other hastily put together material, including the historian's knowledge of some chemical detail.

Is Mr. Sarton speaking as historian or as scientist when he remarks that the fundamental difference between the two disciplines is revealed in the way they grow? For him, historical knowledge grows slowly and precariously because of the recurrence of discredited errors; for him, our knowledge of ancient Greece while expanding, does so at an ever smaller rate. Is that the whole story? The discovery not only of new materials but of altogether new types of materials is compelling a new interpretation of ancient Greece, an interpretation that no historian steeped in literary sources alone would have accepted. Moreover, when to the different materials is added the consideration that each generation will approach ancient

Greece with a different point of view, it may be argued that Mr. Sarton's generalization requires qualification. After all, many an ordinarily intelligent layman has said exactly the same about science as Sarton says about ancient Greece. To be sure, historians have even said, as George B. Adams, a critical and thorough medievalist, stated with respect to feudalism, a generation ago, that certain periods of the past are fully illuminated. How woefully they missed the mark! So, too much criticism need not be directed at Sarton's generalization on this particular beyond the suggestion that historians frequently say the same about science.

In fact, Sarton offers the best criticism of himself though perhaps he holds up the mirror unconsciously. After insisting that in contrast to history science is being constantly revolutionized, and that the chemistry of today is quite different from that of the eighteenth century and the chemistry of the twenty-fifth century will be totally unlike that of today, while the history of the eighteenth century tends to remain the same yesterday, today, and evermore, he clearly reveals a confusion of values. The eighteenth century is constantly undergoing re-interpretation. Some leaders of opinion at that time regarded themselves as living in a wondrously enlightened age, as being dreadfully modern, as owing no allegiance to the past; the mid-nineteenth century certainly had a different view about its predecessor, as Carlyle showed with his fulminations against atheism and immorality. Now the pendulum has recently swung back and the eighteenth century is viewed favorably, although most intelligent historians realize that it was no age of reason, that no century which matched John Wesley against David Hume, or Kant against Voltaire can be too simply regarded. Moreover to take another item, it is only now beginning to percolate through historical writing—what common sense should have indicated long ago—that to speak of the Industrial Revolution in the eighteenth century is ridiculous. Yet non-historians give to the Industrial Revolution and other spurious periodizations a transcendent unity and meaning which cannot be found on closer examination. Again, because in the study of history method and objective are inescapeably fused, the examination of the past will always have vitality and growth. There will always be controversies as to methods of approach and the conclusions that may be drawn. The facts do not speak for themselves, or rather they do, and the response to them is inevitably highly subjective. Thinking about these facts, the materials, and the method of attacking them is not only necessary; it is inevitable. Despite both neces-

sity and inevitability, however, this thinking at times seems conspicuously lacking.

The scientist, says Sarton, can never relax but must both learn new things and unlearn old. Why not the historian? Many a historian in lecturing to his class realizes that he has said things, not merely to a pedagogical end, that conform neither to sense nor to knowledge, and he changes his approach accordingly. On the other hand, he may so put a generalization that it opens up a new meaning and he is impelled to search for substantial proof. In either case he is learning. When Sarton says that history is unprogressive in contrast to the growth of scientific subjects which is "unpredictable, luxuriant, and sometimes explosive in its intensity and explosiveness," he is in part liable to the charge that he directs against the scientist who looks upon history as exceedingly easy and "he does not realize that he is merely despising his perverted image of it." Again, it is the scientist speaking when Sarton observes that "under the healthful influence of geological and prehistoric research, some historians have now become full brothers to naturalists." This is no compliment, for he conceives both the historian and naturalist as collectors, the first a collector of scientific ideas, the second of insects, plants or rocks. Assuredly, the historian may come to appreciate the value of precision from the naturalist, but precision like patriotism is not enough.

The task of the historian of science clearly assumes an exacting training. Probably, and here we are not treating the ideal, the historian of science must concentrate on procedure and technique rather than on substance. Likewise he must ever keep before him the danger of reading modern meanings into ancient writings. Sarton's insistence upon the historian's obligation to know more of modern science in order the better to appreciate earlier science carries within it no little danger, though the truth of such a position cannot be gainsaid. Some investigations into the history of medicine have revealed to what extent earlier science can be distorted when the "idol of present-mindedness" shapes the attitude toward the past.

The suggestions as to training are of interest, and to the historian they sound feasible. The historian of science should, says Sarton, make a special study of (1) the development of one branch of science, (2) the development of science and learning during a special period, and (3) the development of science and learning within a certain country. On these foundations it will be possible to build the substantial monographs necessary to a sound and fuller knowl-

edge of the subject. The necessity of these monographs becomes apparent from the general textbooks, and if we had them we could improve the textbooks which in turn would stimulate more monographs. The task of providing these monographs is, however, most difficult both because of the difficulty of the subject and the scarcity of workers trained or otherwise. This scarcity causes no little anguish to Mr. Sarton to whom science is the backbone of culture, and he would gladly see some of the legions of historians working in other fields transfer their energies to the history of science. This would be possible because, while some knowledge of science is highly desirable, a good deal can come from a penetrating intelligence trained in historical technique. The historian of science after all has to spend only part of his time with the ideas *per se*; he must appreciate what men have thought of those ideas. "Ptolemy's works are one thing, their tradition quite another, both indispensable."

This leads to the necessity for emphasis upon the fact that the historian of science needs a profound knowledge of history as well as a specialized knowledge of science. By such means he may evolve a sense of values that applies to science as well as to historical evolution. History no less than science enhances criticality. Too much for the scientists' good has been written about the objective, rational approach of the scientist. We need not take advantage of the existence of many pseudo-scientists or small-time scientists to point out the absurd pretension to objectivity and unique penetration. We need only recall the statement of one of the most prominent of American scientists, a Nobel prize winner, Robert Millikan, who, in pleading that mankind adopt more the attitude of the scientist in solving its problems, recalled that when he wished to know something about current politics he consulted the experts. When laymen wanted to know something about science they ought, he recommended, to consult the experts in the science. That was very plausible until Mr. Millikan revealed the experts in politics whom he consulted, and they turned out to be Mark Sullivan, David Lawrence, and Walter Lippman. Mr. Millikan's intentions were perhaps excellent but as soon as he revealed his conception of "experts" he made himself ridiculous. What would he say if he heard an historian declare from a platform that when in need of scientific advice, he, the historian, consulted such "experts" as Wiggam and Brinkley? Perhaps, with all his training in objectivity and experiment, the scientist is more naïve about non-science than the historian about science. Perhaps, for that reason the historian of

science needs an even greater knowledge of and training in history than Mr. Sarton is ready to concede, although he is extremely generous in this matter. Perhaps, again, it is no easier to work up that knowledge of the past needed by the historian of science, than to work up the knowledge of science. History too is a rigorous discipline, demanding a great deal of critical thinking. Like the common cold, which everybody gets and no one seems to know a great deal about, history is taken too much for granted.

Because history does demand a penetrating intelligence, it admittedly cannot stand merely on technique—though many writers and investigators have tried to make it do so. Sarton's remarks on technique apply readily to the historical discipline. He quotes with approval the shrewd observation of Davis and Nelson (*Elements of Statistics,* p. 334) "that a statistical conclusion is no better than the judgment of the statistician who produced it. Knowing what tool to employ is just as important as knowing how to employ it. The second can be taught, but the first must be learned." This observation calls to mind the case of a statistician who quite smugly undertook to relate the history of American political parties in terms of statistics. He reached an impasse, however, at 1896 when his figures led him to zero. He had a Populist party before 1896, he had a Populist party after 1896, he didn't have any in 1896. Most distressing! And his statistics ("the only fit tool for everything") did not reveal to him what a high school text would, namely, that in 1896 the Populists took over the Democratic party.

This clearly illustrates Sarton's concluding judgment that "much intellectual mediocrity can be and actually is concealed by some technique sufficiently recondite to discourage outside criticism." Pedantry is always with us, and to the unwary appears in the guise of creative scholarship. The difference between the two lies in the power of selection. Both historian and scientist must choose. History, we repeat, is everything that ever happened, but no wise man believes that by setting down everything that ever happened he has written history. "He must choose and choose and choose again." Technique will carry him along more easily but he must be on the way and know where he is going. By this means he may avoid pedantry. But—and isn't it too bad that cold print cannot possibly convey the gravity of this question—what shall we say of or to a man who on behalf of two diciplines, history and science, in many ways quite diverse but still having certain common objectives, urges his listeners to go no farther than the facts allow, and yet assumes that "some

men are born pedants . . . just as some people are born hypocrites?" We shall say, "Thank heaven, he's human." He has taken, without arrogance, a far larger sector of the province of knowledge, than others with less humility. If he nods now and then, it is no more than the failure to dot an "i."

CHARLES F. MULLETT.

Department of History, University of Missouri.

GURNEE, HERBERT, *Elements of Social Psychology.* New York: Farrar and Rinehart, Inc., 1936. Pp. xi+467.

Two books are selected by Gurnee as having been most profitable to him in writing his text; namely, Allport's *Social Psychology* and the Murphys' *Experimental Social Psychology.* Of the two, the latter book seems to be the model for *Elements of Social Psychology.* In fact, apart from abridgements of selected investigations appearing recently and the inclusion of several chapters evidently dictated by markedly personal inclination, and, of course, with allowance made for interpretations necessarily specific to the author, we find startlingly little in the *Elements* not available in the summaries brought together in the aforementioned *Experimental Social Psychology.* This degree of identity is inevitable, since Gurnee has attempted a reduction in the number of thumb-nail sketches of researches rather than a different mode of approach to the principles of social psychology. He states in his preface, "It is unnecessary and pedagogically unwise, it seems to me, to bewilder the student with a conglomerate array of abridged data, when a few crucial studies will tell him just as much and often more." Whether we have been afflicted earlier with "a conglomerate array of abridged data" is a moot question. Perhaps the author has projected imaginatively the dire effect upon the student resulting from extending summaries of inductive studies. Yet upon occasion seemingly we find pleasure with Freud in discovering errors, even if they are of omission. In spite of Gurnee's gracious acknowledgment of indebtedness to the Murphys, and of his liberal borrowing likewise frankly recognized by specific notation within the body of the text, we find him omitting their names from the index of authors. Nevertheless he who knows both texts, the *Experimental* and the *Elements* cannot fail to note close parallels.

Consideration of the extent to which the author has saved the student from bewilderment may be deferred until topics or fields covered in the text have been indicated. The term fields seems most descriptive, for there is no plot, no sustained principles. Transi-

tions from chapter to chapter are often abrupt. Within chapters, however, there is evidence of careful attention to logical arrangement of materials; and the analyses of behavior into component parts or the emphases upon physiological substrata and the breaking up of situations into less complex patterns enhance the unity achieved. That readers with serious concern about understanding social problems will be dissatisfied to focus narrowly upon the contents of each chapter can hardly be doubted. They seek the implications of researches for the understanding of behavior as it occurs in society; they are presented largely with elements of behavior. The fourteen chapters survey the following fields: methods of investigation and of interpretation used in social psychology; learning, motivation, and emotions and their part in the social aspects of the individual; temperament and personality; the perception of personality traits; and the development of verbal activity. These are followed by chapters dealing with suggestion, social attitudes, group behavior, and social misconduct; finally, two chapters, one concerned with aesthetic activity and the other with religious activity, complete the series.

Where boundaries are tenuous, personal preferences assume major significance in setting limits to textbook content. At least three chapters in this text enjoy novelty. One of them, entitled "The Perception of Personality Traits" deals with systems of character analysis such as palmistry, phrenology, and physiognomy. Since concern is with the baseless assumptions of these systems and with the negative evidence issuing out of selected researches, the chapter heading is misleading. In so far as the emphasis is upon assumed signs of personality traits and the refutations of these signs and not upon the social implications of the systems of character analysis, the reviewer feels that the chapter adheres to rather than develops with the book.

The chapter entitled "Social Misconduct" may likewise be said to be an innovation. It is divided into two major sections, one of which is almost exclusively based upon the investigations of Hartshorne and May into honesty and deceit, and into self-control. Five pages of this section are concerned with relationships derived from the measurement of coöperation and generosity. These materials definitely do not belong to the category "Social Misconduct." It is regrettable that the tagging on of excerpts dealing with "Service" should mar an otherwise well-organized chapter. The author has summarized excellently, in the second section, the data relating to the causes of crime and delinquency.

It is doubtful if the third chapter, having novelty for a textbook

in social psychology, can aid the student significantly in comprehending social behavior. Rather the chapter entitled "Aesthetic Activity" promises greater usefulness to a beginner studying theories of aesthetic appreciation. Emphasis is primarily upon factors underlying this appreciation. Considerable space is devoted to the qualities of art objects: qualities such as form, unity, rhythm, movement, balance, and proportion in the static arts, and rhythm, tone qualities, and form in music. Here, as elsewhere, the author loses sight of social relationships; concern with sensory discrimination, however, serves to emphasize heavily the loss of contact with interacting individuals.

Perhaps it is inevitable that the compilation of data from a variety of sources often having barely perceptible relationships should permit but little generalization. Perhaps generalization is undesirable in a field where until recently few attempts had been made to temper facile explanations by factual data. Yet there remains a conviction that the author has not envisaged the social applications inherent in many of the better controlled experiments in the field. He has assembled many excellent studies, or shall we say he has utilized them selectively to support analyses, but as surely has he left for the student the more difficult problem of generalization. This compliment to the student is seldom earned. He needs more than a minimum of guidance before he can grasp the wider significance of controlled investigations. A knowledge of experimental studies as extensive and intimate as that displayed by Gurnee imposes the privilege of making interpretations of social behavior extending beyond the confines of psychological experimentation. At least hypotheses and theories have proved spurs to further research in other branches of science. In addition, though not different from generalization, it would have been desirable to have emphasized the fact that a large proportion of the researches involve selected samples of a highly selected segment of society, namely the willing coöperators and the unwilling draftees within the American college population.

Of particular merit are the three chapters dealing with methods of social psychology, the learning of social habits, and with suggestion. It is commendable that the author has acquainted the student with the techniques for collecting data and with the grosser of the pitfalls inherent in several of the methods. His adherence to the principles of conditioning in the learning of social habits is likewise praiseworthy and the discussion of imitation in this chapter is excellent although too brief. It is in the chapter on suggestion

that the author has permitted himself the privilege of applying to social situations certain principles derived from research. The attempt heightens comprehension as well as the significance of the researches covered.

For those instructors interested in having students master factual data which temper undue speculation within social disciplines, Gurnee's *Elements of Social Psychology* should serve admirably. There still remains the question regarding the ability of students to assimilate a large volume of data within the short time allotted to college courses. But this text profits from careful selection of available experimental studies. It promises not to tax the student who finds himself inadequately prepared for the more extensive and yet more condensed discourses and compilations found in a larger volume, asserted by the reviewer to be the model. If it seems desirable to escape the consequences of frank discussion of social problems viewed in the light of psychological research, and unfortunately such discussion is not always tolerated nor cannot be within certain academic walls, undoubtedly this text offers the way out with its emphasis upon elements of social behavior. Or, if the instructor is willing to discuss psychological aspects of social interaction while his students are given rein to build up a knowledge of elementary principles and a body of facts, then the text has marked value. Concerning the problems arising from the interplay of individuals in a complex society, problems barely touched upon or rarely mentioned where attention is directed to elements of social behavior, we might permit the author to repeat the closing sentence of his text, "Of these we appear at times to be signally neglectful."

CHARLES BIRD.

University of Minnesota.

LAPIERE, R. T., and FARNSWORTH, P. R., *Social Psychology*. New York: McGraw-Hill Book Company, 1936. Pp. xii+484.

Social psychology has long remained the no-man's-land between laboratory psychology and more or less descriptive sociology. Results of efforts to write a social psychology from the strict view-point of laboratory psychology have tended to be dull and ineffective. Efforts to write sociological psychologies or psychological sociologies have been criticized as being too largely generalities about society or expositions of the social philosophy of the author. The LaPiere and Farnsworth text is a definite effort to utilize the advantages of both psychological and sociological view-points and to produce a work

which would rest on the accumulated findings of workers in these and even related fields. In doing so the risk is taken, on the one hand, of having in the end a volume acceptable to neither side, or of having, on the other hand, one which may be of interest to both.

In the reviewer's opinion, the authors come nearer to accomplishing the latter desirable objective. They have, first of all, a volume which is above all else crammed full of interesting and teachable subject-matter. They have, furthermore, produced a social psychology in which the subject-matter is predominantly *social* psychology. The reviewer, having had four years of Chicago social science followed by graduate training in straight psychology, has had the growing conviction from teaching social psychology over a decade that that portion of the literature of social science, qualifying as good psychology, has now grown to such proportions that no longer is it necessary to lean so heavily upon experiments in neurology, child psychology, certain types of animal psychology, and the like. Current world trends should constitute a warning to psychologists that they must bring their interests and efforts closer to a solution of society's present problems or find that there may be no society worth studying. LaPiere and Farnsworth bring social psychology close to earth. They do not overlook the sound, experimental contributions to basic theory. In their comprehensive and critical chapter appendices they will all be found, if not directly in the text; but the reviewer is impressed with the fact that in addition are included also valuable contributions made in divisions of social science that are too infrequently included in strictly psychological texts.

The book is divided into five parts. The first is in the nature of an introduction to the individual-society problem, beginning with a brief presentation of theories of society from Plato to Cooley. It is probably necessary to lead the student through this but no doubt relief will come with the second and third chapters which are given over to the psycho-biological, and the social phases, respectively, of human behavior and human nature. The instinct controversy, the hereditary-environment problem, motivation, emotional behavior, and the rôle of learning are well treated. As in Allport, the instinct problem is given sane consideration without requiring the student to labor through all the technical studies reviewed in Murphy and Murphy. The sociologist's stand on the influence of membership in groups, the emphasis on the influence of past societies, and related considerations are presented. Part II takes up the process of socialization, expanding other treatments on the rôle of gesture. The authors

feature the term "symbolic behavior" (actions traceable to social origins) and give considerable attention to symbolic behavior in speech. Non-symbolic behavior, first through people and second through ideas of people, is developed at length. As a concept to replace imitation they introduce a development of the idea of the "social model" and later on of the stereotype. An excellent chapter on the dynamics of social adjustments completes this section. The human personality comes in for extended discussion in Part III, introducing "personality stereotyping" and the description and measurement of personality through attitudes, motives, wishes, and interests. The next section follows along this same vein with the emphasis upon personality differentiations, introducing both normal variations and abnormal personality types. The final section entitled "The Situational Nature of Social Behavior" is a catch-all for various descriptive aspects of social behavior usually featured in sociologies and the more descriptive types of social psychology. The content includes such matters as the institutional basis of behavior recently developed by Allport, leadership situations, propaganda, and crowd phenomena.

As a teachable text, the reviewer is convinced that it will prove to be a most excellent addition to the literature of social psychology. Much of the book's content appears to fit in naturally with the comprehensive outline that the reviewer has developed in the course of his teaching, which is probably similar to that used in any standard course. The illustrations are couched in familiar terms and refer frequently to situations found in the experience of students themselves. The style is lucid and direct. Perhaps the most praiseworthy aspect of the text is the inclusion at the end of each chapter of a comprehensive appendix often running into eight or nine pages, geared to the text by footnote references, and supplying source material, experiments, interpretative comments, and bibliography.

NORMAN C. MEIER.

University of Iowa.

HOLMES, S. J., *Human Genetics and Its Social Import.* New York: McGraw-Hill Book Company, 1936. Pp. viii+414.

While the chief stress of this book is upon the facts of heredity and their relation to the problems of social amelioration, several chapters deal quite specifically with psychological data. As one might expect, these chapters concern such topics as nature and nurture in the determination of psychological differences, heredity in relation to

mental defect and disease, and the relation of genetics to the psychology and sociology of crime and delinquency. About one-fourth of the book is devoted to an elementary discussion of the mechanisms and phenomena of inheritance. More than one-half concerns such topics as choice in mating, birth and death rates, biological effects of war, population growth and mobility, inbreeding and cross-breeding, and proposed measures for social betterment. Many of these discussions deal indirectly with psychological facts and theories.

In considering psychological factors, the author takes cognizance of the large amount of recent clinical and statistical material. The data presented suggest, in many instances, the possible existence of hereditary diatheses, the rôle of the environment being largely a precipitating one. As a causal factor in feeblemindedness, environment is regarded as of little consequence, accounting for only a small proportion of the cases. It is thought to play a more significant part in such anomalies as dementia praecox, manic-depressive insanity, and the like. Even here, however, an "insane diathesis" is believed to be present. Whether or not the insanity shall develop is believed to depend upon the precipitating potentialities of the environment. Focal infection is mentioned, in this connection, as one potent environmental precipitating factor. It seems to the reviewer that, although the array of data from genealogical and twin studies indicates a high incidence of particular psychological anomalies in genetically related individuals, the author does not sufficiently stress the possibility of social transmission. Genetic relationship and the tendency toward similarity of physical and social environment usually go together. The high incidence of dementia praecox in a given family may, for example, conceivably be due to the fact that all members of that family "inherit" a similar social environment. It may, on the other hand, result from an hereditary dementia praecox diathesis. The author feels that it is attributable to both hereditary and environmental factors, but he lays what may prove to be undue stress upon defective genes.

The discussion of nature and nurture in mental development is a well-balanced consideration of the available data and the conclusions are those reached by others who have made an impartial study of the literature bearing upon the nature-nurture controversy. Dr. Holmes says, for example: "Both heredity and environment may cause very great differences in the mental development of human beings. . . . Hereditary factors can make the difference between a low grade idiot or imbecile and the native endowments of the highest type of

genius. . . . Environment can depress or inhibit the development of the mind to any degree consistent with the maintenance of life. . . . To a certain extent nurture can increase intelligence as this trait is measured by any kind of test yet devised. Its power of doing so varies in different individuals, being very slight in idiots and imbeciles and becoming greater as hereditary capacity is increased. . . . The fact that both high and low ability runs in families is due to both hereditary and environmental factors. . . . There is still room for reasonable differences of opinion as to the rôle of nature and nurture in causing unequal degrees of intellectual development."

Genetic as well as environmental factors are regarded as of great consequence in crime and delinquency. The more important data are colligated. The relatively low intelligence of criminals, the fact that delinquency runs in families, and similarities in identical twin delinquents, lead the author to conclude that "evidence which has now accumulated shows quite clearly that like heredity in environments which are as a rule of much the same kind is associated to a rather surprising degree with similarities in criminal careers."

A chapter on the social-problem people deals with the Jukes and Kallikak families, the Tribe of Ishmael, the Hill Folk, and similar groups. His analysis of the data concerning such individuals leads Dr. Holmes to conclude that many social problems are related to defective intelligence. He points out that "the stratum lying above the level of imbecility and below the average level of intelligence, furnishes more than its quota of problems of many sorts."

The following psychologically significant questions are also discussed: Is there a differential birth and death rate for the upper and lower intelligence levels? Does war increase or decrease the psychological and other qualities of the race? Is population growth and mobility selective in its effect upon those of different degrees of intelligence? What type of immigration policy will contribute most advantageously to the average intelligence of the nation? In what way does inbreeding and crossbreeding affect the intelligence and other traits of the individuals involved? What measures will do most to ameliorate social problems? Dr. Holmes brings together the widely scattered data which force at least a tentative answer to these questions as well as to many which are not so directly of interest to psychologists.

The book is very clearly written and it is well illustrated. There is an extensive bibliography and a good index. Suggested readings and a number of stimulating questions appear at the end of each

chapter. The book is intended primarily for courses in eugenics. For psychologists, however, it is a convenient source of material on the topics indicated above. Psychologists will also find it of value for supplementary reading in courses such as genetic, social, abnormal, and clinical psychology.

NORMAN L. MUNN.

George Peabody College for Teachers.

BOOKS RECEIVED

BROWN, W., *Mind Medicine and Metaphysics.* London: Oxford University Press, 1936. Pp. viii+294.

FISHER, V. E., *Auto-Correctivism. The Psychology of Nervousness.* Caldwell, Idaho: The Caxton Printers, Ltd., 1937. Pp. 337.

HARDY, M. C., and HOEFER, C. H., *Healthy Growth.* Chicago: University of Chicago Press, 1936. Pp. xii+360.

JONES, L. M., *A Factorial Analysis of Ability in Fundamental Motor Skills.* T. C. Contr. to Educ., No. 665. New York: Bureau of Publications, Teachers College, Columbia University, 1935. Pp. ix+100.

KELLY, H. J., *Anatomic Age and Its Relation to Stature.* University of Iowa Studies, Studies in Child Welfare, Vol. XII, No. 5. Iowa City: University of Iowa, 1937. Pp. 38.

LATOUR, M., *Premiers principes d'une théorie générale des émotions.* (Nouvelle édition revue et augmentée: *Observations complementaires.* Première, deuxième, et troisième série.) Paris: Félix Alcan, 108 Boulevard St.-Germain, 1936. Pp. 27, 36, and 80.

LEVINE, A. J., *Fundamentals of Psychologic Guidance.* Brooklyn: Educational Monograph Press, 1936. Pp. 96.

LUNDHOLM, H., *The Psychology of Belief.* Durham, N. C.: Duke University Press, 1936. Pp. vii+245.

RICHMOND, W. V., *Personality: Its Development and Hygiene.* New York: Farrar and Rinehart, 1937. Pp. xvii+279.

RIVLIN, H. N., *Educating for Adjustment.* New York: D. Appleton-Century Company, 1936. Pp. xiv+419.

SARTON, G., *The Study of the History of Science.* Cambridge: Harvard University Press, 1936. Pp. 75.

SCHONDORFF, A., *Grundlegung einer Volkswissenschaftlichen Lehre vom Menschen.* Leipzig: S. Hirzel, 1936. Pp. 68.

NOTES AND NEWS

THE First International Congress of Child Psychiatry will be held in Paris, July 24 to August 1, 1937. Dr. Wm. Healy of the Judge Baker Foundation, Boston, is the honorary president from the United States. Dr. G. Heuyer is the chairman of the committee on organization. Communications should be addressed to Dr. Léon Michaux, General Secretary, 74 Boulevard Raspail, Paris (VI). Specialists from various countries have been asked to present papers, which will be followed by discussion by members of the congress. Members who wish to participate in the discussion should register before the opening of the congress. Subjects for discussion are: July 24, *The Neuro-physiological Basis of Child Psychiatry;* July 25, *Conditioned Reflexes in Child Psychiatry;* July 27, *Educational Methods Suited to the Intellectual and Character Difficulties of the Child;* and July 28, *Mental Debility as a Cause of Delinquency in Children and Juveniles.*

THE Seventh English-Speaking Conference on Maternity and Child Welfare will be held in the Great Hall, British Medical Association House, Tavistock Square, London, W. C. 1, June 1–3, 1937, under the presidency of Sir Kingsley Wood, minister of health. Among the sessions of the conference is one arranged by the Child Guidance Council, the Nursery Schools Association, and the National Society of Day Nurseries, which will deal with the future of preventive psychology in relation to maternity and child welfare work. Those wishing to contribute papers to this session should communicate with the secretary of the Child Guidance Council, Davis F. Robinson, Woburn House, Upper Woburn Place, W. C. 1, London, England.

PROFESSOR LEE EDWARD TRAVIS has been appointed head of the department of psychology at the State University of Iowa. He will succeed Dean-Emeritus Carl E. Seashore on July 1. Professor Seashore will continue as research professor.

DR. ARTHUR G. BILLS has been appointed head of the department of psychology at the University of Cincinnati beginning with the school year 1937–38.

DR. CARROLL C. PRATT, lecturer in psychology at Harvard University, has been appointed professor of psychology at Rutgers University beginning with the school year 1937–38.

DR. CALVIN HALL, assistant professor of psychology at the University of Oregon, has been appointed head of the department of psychology at Western Reserve University, beginning with the school year 1937–38.